Governance through Shame and Aspiration: Index Creation and Corporate Behavior in Japan

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Abstract

We study how a stock index can affect corporate behavior by functioning as a source of prestige. After decades of low corporate profitability, the JPX-Nikkei400 index was introduced in Japan in 2014. Each year the index selected 400 large and liquid firms deemed to be best-performing in terms of profitability; membership is considered highly prestigious. We document that index-inclusion incentives have led firms to increase return on equity proportionally by 41% on average, via higher margins, efficiency, or shareholder payouts, depending on where they had slack, but not by changing investments or accruals. These incentives are driven by the prestige associated with the index, rather than capital-market benefits. Back-of-the-envelope estimates suggest that the index-inclusion incentives accounted for 16% of the average increase in aggregate annual earnings over our sample period and 20% of the growth in aggregate market capitalization. These findings shed light on a novel mechanism by which longstanding corporate behaviors can be transformed.

Keywords: JPX-Nikkei 400 index; Corporate governance; Japan; Return on equity; Capital efficiency; Corporate norms

JEL: G18, G34, G38, G41, L51, M14, M52

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1 Introduction

Pursuit of prestige and social status is central to human motivation (e.g., Kostritsky, 2013; Posner and Rasmusen, 1999). Some scholars have argued that the allocation of prestige is vital to a properly functioning market economy (Zingales, 2015), and that consideration of prestige must be central to a complete understanding of economic and corporate behavior (e.g., Williamson, 1963; Hayek, 1967).

These propositions imply that prestige could be actively employed as a tool to influence managerial and corporate behavior (Miller and Prentice, 2016). Since Jensen and Meckling (1976), however, the corporate-governance literature in economics, finance, and accounting has largely focused on the role of formal contracts and financial incentives. Because increasing scrutiny of executive compensation could constrain policy makers and boards in the use of these tools in the future, they may need to become more attentive to non-pecuniary approaches to motivating managers and governing firms, including social prestige.

This paper studies how membership in a stock index can serve as a source of prestige, which can in turn motivate managers and influence corporate behavior. Specifically, we study the impact of firms' incentives to be included in the JPX-Nikkei 400 (JPX400), a prestigious new index designed to select and showcase the 400 top-performing Japanese firms in terms of profitability, capital efficiency, and corporate governance. The index premiered in 2014 as a response to Japanese policy makers' concerns that poor capital efficiency, and a culture of de-prioritizing shareholders, had contributed to Japan's decades of economic stagnation. We find that firms' desire for inclusion in this index had powerful effects on improving corporate profitability and capital efficiency, as measured by ROE, and that this incentive was primarily due to a concern for prestige.

The JPX400 selects its membership each year using a composite score based on operating income, ROE, and market capitalization. We focus on firms' responses in ROE, because this measure is the most heavily weighted determinant, the most directly controllable by managers, and directly related to policy makers' goal of increasing capital efficiency. Because firms ranked near the margin of index inclusion are most likely to see their inclusion status change as a function of their performance, we hypothesize that they experience the strongest incentives generated by the index.

To study how firms behave in response to index-inclusion incentives, we exploit the variation in these incentives and examine how they are related to firms' ROE responses. Identifying firms' incentives would ideally require knowing firms' relative ranks in the composite score, information that is not disclosed by the Japan Exchange Group. But because selection for the JPX400 uses a transparent algorithm based on publicly available information, we were able to synthetically replicate these rankings. Our synthetic rankings predict actual JPX400 membership with a high degree of accuracy and explain the variation in the likelihood of index-inclusion across firms.

We employ a difference-in-differences (DID) design using these synthetic ranks. Treated firms are defined as those with synthetic ranks near the inclusion threshold (ranks 301–500 in our main specification); controls are defined as those similar firms with a much smaller probability of gaining inclusion in the index (firms ranked 501–800). Our DID compares the difference in outcomes between these two groups in the post-period (selection years 2014–2016) with their difference in the pre-period (2010–2012).[1] A unique feature of this design is that, unlike traditional DID designs, a firm's treatment status varies over time: a firm's ranking, and thus its distance from the index-inclusion threshold, varies year by year. Thus

[1] We exclude the year 2013, since firms lacked sufficient time to respond in the first year of the index's implementation. Nevertheless, including this year does not change our results qualitatively. The index and its implementation are described in detail in Section 3 and Figure 1.

our research design in effect combines multiple "experiments" in the post-JPX400 period with multiple placebo "experiments" in the pre-period to infer the effect of JPX400-inclusion incentives.

We document three main empirical findings. First, the firms closest to the JPX400-inclusion threshold achieve differential and economically significant increases in ROE. We estimate that these firms improved ROE by 2.4-percentage-points, a 41% increase relative to the pre-period mean for treated firms. A battery of tests supports the hypothesis that this effect is driven by index-inclusion incentives—that is, by the firms' efforts to improve ROE in order to be included in (or to avoid exclusion from) the index. In particular, we document that the ROE increases (a) do not appear to be driven by differential trends, (b) are not due to the realized consequences of inclusion in the index, and (c) are declining in a firm's distance from the threshold of inclusion.

Second, we show that it is the prestige associated with the index, rather than the benefit associated with the cost or availability of capital, that primarily motivates treated firms to improve ROE. We do so by comparing the ROE response of Nikkei225 firms—whose high public visibility makes for especially strong "shame" due to exclusion, but smaller marginal cost-of-capital benefits—to the ROE response of non-Nikkei225 firms, which would derive larger cost-of-capital benefits from inclusion.[2] Using a modified DID design and a difference-in-difference-in-differences design, we show that the ROE response is substantially larger among Nikkei225 firms. This finding is consistent with the hypothesis that it is the prestige of index inclusion, rather than capital-market benefits, that primarily motivates treated firms to improve their performance.

[2]The Nikkei225 is Japan's leading stock market index and the oldest stock index in Asia. It consists of Japan's most highly respected and prestigious firms across 36 industries, as selected by Japan's top financial publication, *Nihon Keizai Shimbun*.

Third, we show that treated firms improve their ROE by means of improved margins, asset efficiency, or shareholder payouts. We find evidence that firms improve margins by cutting discretionary expenses like research and development. However, we do not find evidence that treated firms improve ROE by earnings management, cutting capital investments, or reducing employment or average pay. Our findings suggest that treated firms exploit the channels where they have the most slack to improve their ROE: treated firms with below-median profit margins differentially improve their margins; those with below-median asset turnover improve efficiency; and companies with above-median cash-to-equity ratios increase shareholder payouts. Though the effects on profit margins and asset turnover are economically meaningful, the effect on shareholder payouts is economically small. We also find that the treated firms' ROE improvements drive higher market valuations.

Overall, our findings suggest that the JPX400 index was quite successful at leveraging prestige incentives to improve shareholder equity efficiency at Japanese firms. The estimates also imply that, in the aggregate, the JPX400 made a significant contribution to the overall economy. Our back-of-the-envelope estimates suggest that introduction of the JPX400 increased aggregate net income by JPY1.2 trillion per year, a figure that represents a 8.9% increase relative to the pre-period average aggregate income of Japanese firms or 16% of the change in average annual aggregate net income from the pre-period to the post-period. Moreover, we estimate that the incremental total earnings attributable to the JPX400 represent a 6.9% increase in Japanese market capitalization, accounting for 20% of the overall market capitalization growth from June 2014 to June 2017.

Our findings contribute to the corporate-governance literature in two ways. First, we show that prestige incentives—both aspiration to acquire prestige and shame at loss of prestige—can powerfully shape how organizations and managers behave. We add to a small body of literature on the importance of prestige incentives to corporate executives (e.g., Avery, Chevalier, and Schaefer, 1998; Raff and Siming, 2017; Focke, Maug, and Niessen-Ruenzi, 2017), and contribute a fresh perspective to the governance literature, which has focused on pecuniary incentives (e.g., Bebchuk and Fried, 2003). We also show that, as a source of prestige, a stock index can shape incentives and thus influence corporate behavior. These insights could be valuable to policy makers seeking to create non-pecuniary channels of influence on corporate governance; such channels may be particularly useful in the context of increasing pressures, in the United States and abroad, to limit executive compensation, and in developing market contexts were there may be greater constraints to contracting.

Our findings also contribute to the index-inclusion literature. Most prior literature on indexes has examined how index constituents behave and perform *in response* to inclusion in an index (e.g., Shleifer, 1986; Harris and Gurel, 1986; Dhillon and Johnson, 1991; Lynch and Mendenhall, 1997; Chen, Noronha, and Singal, 2004; Doh, Howton, Howton, and Siegel, 2009). In contrast, we study is novel in highlighting how firms behave and perform in order *to achieve inclusion* or *to avoid exclusion*. In other words, our research focuses on index-inclusion-incentive effects rather than the index-inclusion effects. We also present a novel empirical strategy for estimating the effects of index-inclusion incentives more generally. Our DID research design could be adapted to estimate the effects of other indices with transparent inclusion criteria, using publicly observable performance measures.

Finally, our work informs the the growing interest in using stock indexes to shape corporate behavior and the standards of corporate governance. For example, two of the largest index providers–S&P Dow Jones and FTSE Russell—announced in July 2017 their decisions to exclude certain firms with multiple share-class structures from their indexes. Despite these significant moves, empirical research has not established whether and how stock indexes can be effective in shaping standards of corporate behavior. Our analyses highlight an important, and perhaps surprising, reason why indexes can motivate and influence changes in corporate behavior: managers' concerns for prestige.

The remainder of the paper proceeds as follows. Section 2 discusses the literature on prestige and human behavior and provides background on the JPX400 index. Section 3 explains our research design and identification strategy. Section 4 describes our empirical analysis in detail. Section 5 discusses the overall effect of the JPX400 and provides back-of-the-envelope estimates on aggregate earnings and market-capitalization impacts. Section 6 concludes.

2 Background

This section discusses the institutional, economic, and corporate governance context in Japan. It also provides a review of the literature on prestige incentives. Finally, it provides the background on the JPX-Nikkei400 index.

2.1 Governance and Reform in Japan

In post-war Japan, managers prioritized the interests of employees, suppliers, customers, and strategic stakeholders over those of shareholders (e.g., Yoshimori, 1995; Ito, 2014). Shareholders developed strong formal legal rights over the years, but Japanese corporate culture assigned less prestige to the pursuit of shareholder value than to guardianship of other stakeholders' interest, limiting shareholders' *de facto* power. By the early 2000s, shareholders' interests still had little weight and legitimacy in managers' eyes, and activists were rarely successful at convincing even fellow shareholders to support initiatives to boost payouts and efficiency. These norms persisted despite the government's efforts to improve the formal corporate-governance regime (e.g., Allen, Carletti, and Marquez, 2015; Kato, Li, and Skinner, 2017). Many policy makers in Japan cited the country's stakeholder-oriented culture as an explanation for its lagging corporate-capital efficiency—its low return on equity (ROE) and return on assets (ROA)—and decades of economic stagnation.[3]

Many policy makers came to believe that improving Japanese corporations' capital efficiency, by increasing dialogue with and focus on investors, was vital to reviving the economy, an urgent concern in light of Japan's looming problems. As the Ito (2014) report notes:

> Japan faces a rapidly aging and declining population and a decreasing stock of labor and household financial assets. Japan has no room to waste its limited resources and capital. Japan must effectively leverage the resources. . . . In other words, increasing capital efficiency in the broadest sense is crucial from the perspective of Japans survival. Japanese companies—as a critical source of value creation—must strive to increase capital efficiency through their dialogue with investors, and contribute to the accumulation of a broad range of capital stock that will serve as the foundation for future economic prosperity.

These goals and concerns were reflected in the Abe's administration's "third arrow" of structural reforms, a large fraction of which focused on increasing Japanese corporations' accountability to and focus on shareholders.

2.2 The JPX-Nikkei 400 Index

In 2014, in response to these calls for reform, the Japan Exchange Group (JPX) and Nikkei Inc. launched a prestigious new stock index, the JPX-Nikkei 400 (JPX400). The JPX selected and showcased the top 400 firms in Japan, as ranked on a measure of profitability,

[3] For example, Chart 3 in the Ito report Ito (2014) cites an average ROE of 5.3% among Japanese firms in the TOPIX500 in 2012, or roughly one-quarter of the average ROE among U.S. S&P500 firms and one-third of the average ROE among the firms in the Bloomberg European 500.

capital efficiency, and valuation (subject to meeting certain governance and liquidity criteria); see Section 3 for a full and exact description of the algorithm. Each summer, the JPX reformed the index membership, dismissing firms that no longer made the cut and adding firms that had improved their performance. The index *per se* offered no direct financial benefits to the constituent firms or their managers, but membership in the index was considered highly prestigious; it was colloquially referred to as ”the shame index,” in reference to the experience of firms that were excluded. The JPX400’s prestige was further bolstered by the Government Pension Investment Fund’s (GPIF) decision to use it as a benchmark for its passive investments in 2014.[4] Anecdotal evidence, news reports, and interviews with top managers in Japan all suggested that many managers—at firms that were initially included and at those initially excluded—were motivated to improve their firms’ performance. Many excluded firms aspired to gain entry; many included firms feared the shame of eventual exclusion.[5]

Two features of the JPX’s selection criteria are noteworthy. First, the algorithm is explicit, transparent, and, with the exception of a small number of “qualitative adjustments,” based on publicly available financial data. Thus we can reconstruct firms’ JPX400 ranks, including synthetic (or placebo) ranks for years prior to the existence of the index. As we will

[4]Two 2014 reforms were designed to encourage manager-shareholder dialogue: the Stewardship Code and the Corporate Governance Code. The Stewardship Code encouraged institutional investors to pursue long-term returns and to engage companies in constructive dialogue. The Corporate Governance Code, a “bill of rights” for shareholders, urged companies to respect shareholder rights, improve capital efficiency, engage investors in dialogue on a regular basis, and appoint at least two external directors to their boards. Neither code is legally binding: institutional investors and companies subject to the codes are not required to abide by all of their principles; instead they are required to either comply or explain.

[5]After Amada, a well-established 68-year-old tool-maker and a member of the Nikkei225 index, was excluded from the inaugural JPX400 index in 2014, company president Mitsuo Okamoto announced that the company intended to improve its capital efficiency and shareholder returns and to appoint independent directors in order to gain entry into the prestigious new club. Similarly, some firms that were included in the index, such as Unicharm, announced measures that aimed to cement their status among the elite by further improving ROE (McLannahan, 2014).

discuss in more detail below, the transparency and replicability of the selection algorithm is critical to our research design.

Second, the JPX publicly names and tracks the constituents of the index each year, but it does not publish their underlying ranks. Consequently, the publicity surrounding the index churn each August focuses on companies' inclusion/exclusion status, but never addresses their relative ranks. This scenario motivates our assumption that companies near the margin of inclusion/exclusion have stronger incentives than those whose ranks render them relatively "safe."

The index's creation, and its churn each August, have generated substantial excitement and attention in the media, but we have seen no attempts to systematically estimate its impact. The Japan Exchange Group has been tracking and publishing the index's returns, but for reasons we will discuss in Section 5 the index returns do not appropriately capture its overall effects on the market.

2.3 The Role of Prestige

The desire and competition for prestige are important motivators of human behavior. As early as 1966, Nobel laureate John Harsanyi asserted that "[A]part from economic payoffs, social status (social rank) seems to be the most important incentive and motivating force of social behavior"; Harsanyi included status among the goods being bargained over in formal game-theoretic models of organizations (Harsanyi, 1966). Economic theory models agents as having preferences among bundles of diverse goods, and it seems reasonable to assume that social prestige is one of the goods that humans—and perhaps managers in particular— desire. In a review of research on CEOs, Bertrand (2009) states that individuals are motivated to seek to become CEOs by "the prestige, high social status, and high salaries."

Several scholars in economic fields have argued for the importance of prestige and status in advancing our understanding of corporate and financial behavior. Elster (1989), for example, argues that norms that are "shared by other people and partly sustained by their approval and disapproval" are stronger determinants of human behavior than the "instrumental rationality" that has been the traditional focus of economic theory. More recently, Zingales (2015) frames social prestige as an incentive that emerges via a Hayekian evolutionary mechanism to "fill the gap between (perceived) social and private returns of various activities. For example, fighting the spread of Ebola is an activity with a very high social return, but a very low private return. People who engage in such activity are held in high regard by society." In other words, cultures assign high status to—and thus incentivize people to undertake—activities that generate benefits for society that cannot be internalized, monetized, and/or fully contracted around. Prestige thus becomes a motivator of behavior that is traded off with those goods.

Nonetheless, economic approaches to incentive contracting and corporate governance have tended to focus on the formal, monetary incentives of managers—perhaps because these incentives are easier to measure and model, and because it is difficult in many settings to distinguish the influences of managers' desires for wealth and for social status since the two often move together. For example, managerial "empire building" is often attributed to the nonpecuniary desire to run a large and prestigious firm (e.g., increasing its probability of being included in the Fortune 500); it is also frequently attributed to pecuniary incentives, under the assumption that firm size drives CEO pay. The empirical literature has found mixed results across different time periods and locales. For example, Disney, Bridges, Gathergood, Disney, Bridges, and Gathergoodj (2006) find that, in the United Kingdom between 1981 and 1996, acquisitions increase CEO pay despite reducing shareholder wealth.

In contrast, Avery et al. (1998) finds that the compensation growth of acquiring CEOs does not differ significantly from that of their matched non-acquiring peers, which the authors interpret as evidence that acquisitiveness is driven by the desire for prestige and status.

2.4 Prestige in Corporate Japan

Both anecdotal evidence and research by Japanese scholars suggest that social status and prestige are important in the Japanese corporate environment. Aoki, Jackson, and Miyajima (2007) note that CEOs in Japan are overwhelmingly internally promoted, and argue that they are thus perceived as, and act as, "top employees" of the firm rather than agents of shareholders. The authors characterize the Japanese corporate environment as "relational," i.e., focused on long-term relationships and reputation. Today's Japanese corporate culture emerged in a post-war period characterized by government-led reconstruction and industrial policy; even early in the 21st century, coordination between Japanese corporations and government ministries remained close, and managers saw themselves as guardians of collective "corporate value" rather than shareholder value (Aoki et al., 2007). In 2014 Japanese managers rated shareholders as their fourth most important stakeholders, behind customers, employees, and suppliers.[6] Even more strikingly, not just managers but also *shareholders* in Japan may be focused on non-pecuniary desiderata. One salient example of this comes from the experience of activist shareholders in Japan in the early 2000s. As Buchanan, Chai, and Deakin (2013) document, activist shareholders were frequently able to get open shareholder votes on proposals that cash-rich Japanese firms increase their payouts, and faced no formal structural barriers (such as from corporate charters, classified shareholders, or Japanese courts), but their proposals were simply voted down by investors loyal to management.

[6] "Change for the Better: Corporate Governance in Japan," Schroders TalkingPoint, April 22, 2014, Figure 1, accessed December 5, 2014.

We thus assume that social prestige and status are important determinants of managerial behavior in Japan, and we hypothesize that the JPX400 index can influence corporate behavior by functioning as a source of prestige. Anecdotal evidence, news reports, and our interviews with Japanese managers and investors all suggest that JPX400 quickly became "the" high-status index, and inclusion in the index is considered to be prestigious.

Bringing these various strands together, we frame the JPX400 index as an effort to influence Japanese corporate behavior by assigning social prestige to inclusion in an index based on performance measures of interest to policy makers and shareholders. This initiative was consistent with Japanese policy makers' belief that improved competitiveness and capital efficiency would help revive the Japanese economy. In the terms of the Zingales (2015) framework, the JPX400 represents policy makers' attempt to employ social prestige to direct corporate behavior toward socially beneficial ends.

3 Empirical Design

We study how the JPX400's index-inclusion incentives affect firm performance. Firms closer to the inclusion threshold are most likely to see their inclusion status change as a function of their performance. Thus we hypothesize that, all else equal, firms closer to the threshold of inclusion will differentially improve ROE, the pertinent performance metric that is most readily controllable by managers. We also hypothesize that such an incentive can operate through managers' concerns for prestige—either the threat of loss of prestige (shame) or the desire to gain prestige (aspiration).

Testing these hypotheses requires us to measure firms' index-inclusion incentives ex ante. Our maintained assumption is that, though the JPX does not publish its rankings of firms, managers are aware (at least approximately) of their relative rankings and of their firms' proximity to the threshold of inclusion due to the transparency of the JPX400's selection algorithm.

Thus our empirical strategy is anchored on a synthetic replication of the JPX400's rankings of eligible firms. We first validate these synthetic rankings, then use them as the basis of a difference-in-differences design to infer how index-inclusion incentives affect firms' ROE. These steps are described in the following subsections.

3.1 Synthetic JPX400 Ranks and Sample Construction

We obtain Worldscope data on annual fundamentals (including but not limited to all the underlying variables listed in Table A1), and Datastream data on monthly prices, volume, outstanding shares, and returns, for a comprehensive list of Japanese securities in the period 1990–2017.[7] We omit observations that are missing returns, have an empty "data date" field for fundamentals data, or are duplicated on their Datastream identifier and relevant time indicator. We also merge in an indicator for Nikkei225 membership, constructed using historical updates on constituent firms archived on the Nikkei website.[8]

We then replicate the algorithm used by the Japan Exchange Group to construct the JPX400 index, and employ the resultant synthetic ranks to design our empirical analyses.[9] As Figure 1 shows, the JPX selects a new crop of JPX400 members each year on the last day

[7]Selection of firms for inclusion in the JPX400 was initiated in 2013; we collect data as far back as 1990 in order to perform placebo tests described in detail below.

[8]The set of current constituents and historical changes can be obtained at https://indexes.nikkei.co.jp/en/nkave/index/component. We first construct an annual dataset consisting of all Nikkei225 firms in each year, then match those firms to our baseline sample using their four-digit tickers (Datastream's "Local Offering Code"). Finally, we define a Nikkei225 indicator that equals to 1 if a firm belonged to the Nikkei225 on the date when the JPX400 index was announced. We achieve a complete match for Nikkei225 members in all years.

[9]For the JPX400 selection algorithm and other important dates, see the JPX-Nikkei Index 400 Guidebook, available at http://www.jpx.co.jp/english/markets/indices/jpx-nikkei400/.

of June, using available price and volume data and financial-statement data released prior to April.[10] We use the same information used by the JPX to construct our own synthetic rank.

The JPX400 selection algorithm begins by filtering TSE-listed companies on several criteria. First, it excludes all companies that (1) have been listed on the TSE fewer than three consecutive years, (2) have had negative book value in *any* of the past three years, (3) have had negative operating income in *all* of the past three years, or (4) are in the process of being de-listed.[11] From this pool of eligible firms, the JPX then selects the top 1,200 stocks by "trading value" (price times volume, or the total value of transactions in the stock over the past year). Finally, the JPX then filters down these 1,200 stocks to the top 1,000 stocks by market capitalization.

These 1,000 firms, which we refer to collectively as "the ranked set," are then ranked using the following composite score ($Total\_Score$):

$$Total\_Score_{i,t} = .4 \times ROE\_Rank_{i,t} + .4 \times Op\Pi\_Rank_{i,t} + .2 \times MCap\_Rank_{i,t}, \quad (1)$$

where $ROE\_Rank_{i,t}$, $Op\Pi\_Rank_{i,t}$, and $MCap\_Rank_{i,t}$ are firm i's ranks in the ranked set on 3-year average ROE, 3-year total operating profit, and market capitalization respectively.

Each year's index constituents are chosen on the basis of the highest $Total\_Score$, with one caveat: the JPX reserves the right to make up to 10 "qualitative adjustments" per year based on corporate governance and disclosure-related factors. These qualitative adjustments are not determined by factors that we can observe, but, according to our interview with

[10] As Figure 1 shows, the inaugural year was an exception to this rule. As we explain below, our empirical design excludes the first year (2013) of index selection.

[11] We designate as TSE-listed all companies listed on the First, Second, and Mothers Sections of the Tokyo Stock Exchange and those listed on the JASDAQ.

representatives of Japan Exchange Group (and the empirical evidence presented in the next subsection), they are insignificant. For our purposes, we treat qualitative adjustments as random noise in the index-inclusion rule. We follow the JPX400 selection algorithm precisely, with the exception of the qualitative adjustments, and create synthetic JPX400 ranks for each year from 1994 through 2016.

3.2 Research Design

We utilize these synthetic rankings to test how index-inclusion incentives affect firm behavior. Our main dependent variable of interest is ROE. Of the three components of the index-selection score (equation 1), ROE is the only scaled variable; thus its ranking is the most controllable by managers. The other two components, market capitalization and operating income, are unscaled; thus their variation is largely driven by firm size. Managers may be able to increase firm size via seasoned equity offerings or acquisitions, but these actions would be likely to generate a competing effect on a firm's rankings for the JPX400 by increasing the equity base and, *ceteris paribus*, decreasing ROE. Thus we expect to observe the incentive effects of the JPX400 most cleanly in ROE.

Our main tests compare the ROE of the firms closest to the threshold of JPX400 inclusion—those with synthetic ranks of 301–500—to that of firms outside the threshold of inclusion—those with synthetic ranks of 501–800. We make this comparison in 2014–2016 only, excluding 2013 because the JPX400's inaugural constituents were announced at the end of that year, affording firms only three months to respond to index-inclusion incentives. Next, to assess the index-inclusion effect on ROE, we benchmark this first difference against the baseline difference between firms with synthetic ranks of 301–500 and those with synthetic ranks of 501–800 prior to introduction of the JPX400 (in the years 2010–2012). This second difference accounts for the possibility that the treatment assignment, based on the largely deterministic JPX400 selection mechanism, could be associated with baseline differences in ROE or other determinants of rank. Unlike in traditional difference-in-differences (DID) designs, in our setting a firm's treatment status varies over time: its ranking, and thus its distance from the index-inclusion threshold, varies year by year. Therefore our research design in effect combines multiple "experiments" in the post-JPX400 period with multiple placebo "experiments" in the pre-period to infer the effect of inclusion incentives.

Together, our basic research design is summarized in the following DID specification:

$$ROE_{i,t+1} = \alpha + \beta_1 Treat_{i,t} \times Post_t + \beta_2 Treat_{i,t} + \gamma X_{i,t} + f_t + \epsilon_{i,t}, \tag{2}$$

where $Treat_{i,t}$ is an indicator equaling 1 for firms ranked 301–500 and 0 for firms ranked 501–800 in a given selection year t; $ROE_{i,t+1}$ is a firm's return on equity in the following fiscal year; $Post_t$ is an indicator for the period after the introduction of the inaugural JPX400 constituents, equaling 1 for years 2014–2016 and 0 for years 2010–2012; $X_{i,t}$ is a vector of contemporaneous firm controls; and f_t represents time fixed effects.

The main coefficient of interest, β_1, captures the mean ROE differences between the treatment and control firms in the post-JPX400 period, relative to the differences between placebo treatment and control firms in the pre-JPX400 period. Our identifying assumption is that any baseline differences in future ROE—that is, in the absence of JPX400-inclusion incentives—between firms near the inclusion threshold (treated) and those further from the threshold (controls), if they exist, are stable over time and thus accounted for by pre-period differences between placebo treatment and control firms.

We believe that this assumption is most defensible when conditioned on contemporaneous

ROE, an important predictor of future ROE, because the distribution of contemporaneous ROE can change after the introduction of the JPX400. Thus, our most robust specification—the specification on which we rely most heavily throughout our empirical analysis—includes contemporaneous ROE (or the lagged dependent variable more generally) as a control. Another way to interpret this specification is that its DID estimate (β_1) identifies the treatment effect by comparing the mean differences in firm-level *changes* in ROE between the treatment and control firms in the post-JPX400 period to the mean differences between placebo treatment and control firms in the pre-JPX400 period.

Finally, we note that the DID coefficients produced by this research design represent a conservative estimate of the JPX400-inclusion incentive effect for the treated group of firms. This is the case because our research design uses as controls those firms that are less influenced by index-inclusion incentives (those ranked 501–800)—effectively assuming that they are unaffected by those incentives. To the extent that these firms do respond to some degree to the incentives of JPX400, our DID estimates would be downward-biased.

3.3 Validation of Synthetic Ranks

Our research design relies on a synthetic replication of the JPX400 ranks, which is necessary because the JPX publishes the index constituents but not the underlying rankings. Thus we begin by empirically validating our synthetic rankings and testing whether their variations are meaningfully associated with the probability of JPX400 inclusion (and thus with the true rankings). We estimate the following OLS regression:

$$Actual\_Inclusion_{i,t} = \alpha + \beta Predicted\_Inclusion_{i,t} + \gamma X_{i,t} + \epsilon_{i,t}, \tag{3}$$

where $Actual\_Inclusion_{i,t}$ is an indicator for actual JPX400 membership, $Predicted\_Inclusion_{i,t}$ is an indicator that takes a value of 1 for all firm-year observations for which our synthetic JPX400 rank is less than or equal to 400 and zero otherwise, and $X_{i,t}$ is a vector of contemporaneous firm-level controls. To ensure that our analysis of prediction accuracy includes all false negatives, the regression sample includes all firms with synthetic ranks of 1–2000 (based on the top 2,000 firms in terms of trading value) for the selection years 2013–2015.

Columns 1 and 2 in Table 1 report the results of this analysis. Firm-level controls are incrementally introduced in column 2. Column 1 shows that, unconditionally, a synthetic rank lower than or equal to 400 is associated with a 92% probability of index inclusion. Column 2 adds firm-level controls to assess the extent to which prediction errors, such as those due to "qualitative adjustments," are systematically correlated with firms' fundamentals. We find that *Log Market Cap* alone exhibits a significant relationship with the likelihood of JPX400 inclusion; no other firm attribute—*Book to Market*, *Sales Growth*, *LT Debt to Equity*, or *Cash to Equity*—does so. Because the JPX400 is weighted toward large firms, the positive association between firm size and inclusion likelihood is likely due to the small number of false negatives being on average much larger in size than the full sample of 2,000 firms.

Columns 3 and 4 in Table 1 report results of similar analyses, but instead of $Predicted\_Inclusion$ they use indicators of synthetic rank ranges—1–200, 201–400, 401–600, and over 800—as the main predictors of interest. These tests represent a more granular assessment of whether the variation in the synthetic rank is meaningfully associated with the likelihood of JPX400 inclusion. Because the quintile indicators are exhaustive, these specifications are estimated without a separate intercept term.

The results in columns 3 and 4 show that the probability of JPX400 inclusion decreases with our synthetic ranks. By interpreting the coefficients in column 3, we find that, unconditionally, firms we ranked 1–200 have a 99.1% likelihood of inclusion, and firms we ranked 201–400 have an 87.9% likelihood of inclusion. Lower-ranked firms have low likelihoods of inclusion: firms we ranked 601–800 have a 0% likelihood of inclusion. Again, because our synthetic ranks produce a small number of false negatives, we find a statistically significant but economically small likelihood (0.5%) of inclusion for firms we ranked above 800. The results reported in column 4, which include firm fundamentals as additional controls, are virtually identical to those in column 3. As in column 2, the only firm characteristic significantly associated with the likelihood of inclusion is firm size, because false-negative firms are on average larger.

Collectively, these results show that our reconstructed synthetic JPX400 rankings possess a high degree of validity. That these ranks cannot be improved on by additional controls other than size suggests that our prediction errors are unlikely to cause systematic biases in our main results.

3.4 Summary Statistics

Table 2 reports pre-period summary statistics for the sample of firms used in our main analyses (firms with synthetic ranks of 301–800). The first five columns report the distributional statistics (quartiles, mean, and standard deviation) of covariates for the full sample; the rightmost three columns report the means for the treatment group (ranked 301–500) and the control group (ranked 501–800) and the t-statistics of their differences. Definitions of covariates appear in Table A1.

It is noteworthy that our treated and non-treated firms differ in their means on several variables related to profitability and size, such as *ROA*, *Asset Turnover*, *Log NOA*, and *Log Market Cap*. This pattern is to be expected, since the treatment firms are ranked higher than the control firms, and the synthetic ranks are driven by size and profitability. These differences motivate our inclusion of linear controls for firm characteristics.

Note that, though we could have included firms ranked 1–300 and/or 801–1,000 in our control group, untabulated tests revealed that doing so accentuates the degree of covariate imbalance. Thus our design combines a pre-estimation matching of similar firms with linear controls to account for local differences.

4 Empirical Analysis

This section presents empirical tests of our hypotheses about the effects of the JPX400 on corporate behavior.

4.1 JPX400 Inclusion Threshold and Future ROE

To test the hypothesis that firms closer to the threshold of JPX400 inclusion are more likely to improve their ROE, we estimate equation (2). Table 3 reports estimation results for various specifications of our main DID regression, which compares the forward ROE of firms around the threshold of inclusion (the treated group of firms ranked 301–500) to that of firms under the threshold of inclusion (the control group of firms ranked 501–800).

Column 1 in Table 3 estimates a basic DID specification, without time fixed effects and without any other controls. The DID estimate of the treatment effect—the coefficient on $Treat \times Post$—of 2.8 percentage points is statistically significant at the 1% level, and represents a 48% increase in ROE relative to the pre-period treatment-group mean ROE of 5.85%.

Also noteworthy is the positive and significant (at the 1% level) coefficient on *Post*, an estimate of the secular trend in ROE. The point estimate of 0.018 implies that firms assigned to the control group had a 1.8-percentage-point higher ROE in the post period, or a 35% increase relative to the pre-period control-group mean of 5.12%. In the context of this economically significant *Post* coefficient, the DID estimate can be interpreted in two ways. At one extreme, we can attribute all of the secular trend to the effects of the other contemporaneous governance reforms, such as the Corporate Governance Code and the Stewardship Code. If we do so, our DID estimates suggests that the effect on ROE of being close to the index-inclusion threshold is at least as large as the effects of these reforms. On the other hand, to the extent that part of the *Post* coefficient reflects the effects of the JPX400-inclusion incentives on firms ranked 501–800, our DID coefficient would be downward-biased.

Columns 2-4 present estimates from increasingly robust DID specifications relative to column 1. Column 2 replaces the *Post* indicator with time-fixed effects; column 3 adds industry-fixed effects and linear controls for contemporaneous firm attributes that can can also explain future ROE, specifically *Log Market Cap*, *Book to Market*, *Sales Growth*, *LT Debt to Equity*, and *Cash to Equity*; and column 4 includes contemporaneous ROE as an additional firm-level control. Most notably, the coefficient on $Treat \times Post$ remains similar in size and statistical significance; by contrast, the adjusted R^2 of the regression increases from 2.19% in column 2 to 30.31% in column 4, mitigating concerns about omitted variable biases (Oster, 2017). Interpreting the coefficient in column 4, the most robust specification, we report a DID coefficient of 2.4 basis points, or a 41% increase relative to the pre-period treatment-group mean.

As Section 2 explains, our identifying assumption is that any baseline differences in future ROE between the treated and control firms are stable over time. We conduct two tests of this assumption. The first test assesses whether there is evidence that the differences in ROE between the two groups are changing in the pre-period. Column 5 reports a specification that augments the specification in column 4 with the following additional interaction variables: $Treat_{i,t} \times Year_{2011}$ and $Treat_{i,t} \times Year_{2012}$, where $Treat_{i,t}$ is defined as in equation 2, and $Year_{2011}$ and $Year_{2012}$ are indicators for the selection years 2011 and 2012. These interaction coefficients are insignificant both economically and statistically (at the 10% level), meaning that relative to the baseline of 2010 the differences between treated and control firms do not differ in 2011 or 2012.

We also conduct a second, and more expansive, set of tests based on placebo test regressions going back to 1994. Figure 3 graphs the results of five placebo DID estimates, in which we implement our main regression specification (see column 4 in Table 3) for five sets of 7-year sample periods prior to the introduction of the JPX400. In each year, we rank firms based on the JPX400 selection algorithm and the composite score of equation (1), and create placebo treatment and control indicators as in our main tests. Following the precise setup of our main empirical tests, we take 7-year samples (using year 4 as the post-period indicator), drop year 4 from the analysis, and define the last three years of the sample as the post period; then we estimate the DID specification of column 4 in Table 3. Under our identifying assumption, we expect to find placebo DIDs that are statistically no different from 0. Indeed, Figure 3 shows that none of our placebo DID estimates are statistically different from 0, and that most of the point estimates are close to 0, providing further support for our identifying assumption.

4.2 Establishing Index-Inclusion Incentives

The results of Table 3 suggest that firms near the threshold of inclusion in the JPX400 significantly increased ROE. These findings could be consistent with managers improving ROE in hopes of gaining or maintaining membership in the index, but they could also reflect the *ex-post* consequences or benefits of membership in the index or of some omitted incentives that happen to correlate with the JPX400 ranks.

We run a series of tests to show that our findings are explained by *ex-ante* JPX400-inclusion incentives. Table 4 reports the results of various tests, all based on the baseline specification of equation 2, that employ alternative definitions of treatment and control-group firms, different samples, or alternative measurements of treatment intensity.

We first test for the possibility that the ROE effects are due to consequences or benefits of inclusion in the index. Column 1 in Table 4 reports the estimates from a specification that splits our treatment indicator into two separate indicators: one for firms that are included in the index—that is, firms with ranks within 301–400, and the other for firms that just missed inclusion in the index, ranked 401–500. If *ex-post* benefits of inclusion explained our results, we would expect to see a stronger coefficient on treated firms ranked 301–400. We find, however, that the estimated DID coefficients for the two indicators are nearly identical to each other—0.027 for those ranked 301–400 and 0.024 for those ranked 401–500—and to our baseline DID estimates in Table 3. Both coefficients are statistically significant at the 5% level; furthermore, they are not statistically different from each other, which is consistent with ROE improvement being driven by firms' *ex-ante* incentives to pursue inclusion in or avoid exclusion from the index.

Column 2 in Table 4 provides the results of an alternative test in which we also include firms ranked 1–300 as placebo treatments. If our ROE effects are driven by *ex post* index-membership benefits, we would expect to see a coefficient on *Rank 1–300*$\times$*Post* that is similar in sign and magnitude to the coefficient on $Treat \times Post$. However, we find that the coefficient on *Rank 1–300*$\times$*Post* is (1) statistically no different from zero at the 10% level and (2) statistically different, at the 1% level, from the coefficient on $Treat \times Post$, which remains at 0.023. Thus, firms ranked 1–300 exhibit no differential response in *Forward ROE* as compared to firms ranked 501–800; this pattern is consistent with firms facing stronger incentives to improve ROE when they are nearer to the threshold of index inclusion.

The results in Table 4, column 2, also help to rule out the possibility that our results are driven by unobserved factors that could be a function of firms' JPX400 ranks. If that were the case, we might expect the coefficient on *Rank 1–300*$\times$*Post* to be large and significant. We thus conduct the following cross-sectional placebo test. Because the JPX400 selection algorithm filters down to the top 1,000 firms in Japan by market capitalization and liquidity, we use the next 1,000 firms, those ranked 1,001 to 2,000 by market capitalization, rank them according to equation 1, and re-run our main test. If some omitted factor associated with the algorithm's rank-ordering were driving our results, we would expect the effect to appear in this placebo sample of smaller firms. But the estimated coefficient on *Placebo Group Treat*$\times$*Post* in column 3 indicates no such evidence, consistent with the findings in column 2. We thus conclude that it is unlikely that some omitted factor correlated with JPX400 ranks is confounding our results.

We perform a final test to show that the ROE effects we document are attributable to firms' desires to preserve or attain JPX400 membership. Because firms nearer to the threshold of inclusion are the most likely to see their inclusion status change as a function of their performance, we hypothesize that index-inclusion incentive effects on ROE are stronger for those firms closer to the threshold of inclusion. To capture the variation in treatment intensity, we sort the 1,000 "ranked set" firms into five quintiles based on the negative of the absolute value of their distance from the rank-400 cutoff. The resultant variable, *Quintile(Closeness)*, ranges from 0 to 4 and is *increasing* in proximity to the JPX400 cutoff: higher values represent more intense index-inclusion incentives.

Column 5 reports the results of our estimates using the entire ranked set and this alternative treatment measure. We find a positive and significant coefficient (at the 5% level) on *Quintile(Closeness)*$\times$*Post*. Together with the results in columns 1-4, these findings suggest that the observed effects on ROE are likely driven by firms' desire to be included in the index.

4.3 Incentive Channels: Prestige vs. Capital-Market Benefits

We now turn to *why* firms wanted to be part of the JPX400 index. Our hypothesis is that firms coveted membership in the index for reasons of prestige—the aspiration to acquire prestige or the desire to avoid shame at loss of prestige. In light of the perceived prestige of membership in the JPX400 and its colloquial nickname—the "shame" index—this is a plausible premise. It is also possible, however, that firms coveted membership in the index for reasons relating to capital-market benefits, such as greater liquidity or lower cost of capital (both realistic expectations given that the GPIF promised to track the index and that other institutional investors could be expected to follow suit).

To determine which of these two possible explanations drives the overall treatment effect on ROE, we exploit cross-sectional variation in the treatment response between Nikkei225 and non-Nikkei225 firms. The Nikkei225 is Japan's leading stock index, closely tracked by institutional investors; it consists of Japan's largest, best-established, and most liquid firms across 36 industries. These firms are likely to derive less incremental capital-market benefits from inclusion in an additional stock index than would non-Nikkei225 firms. On the other hand, Nikkei225 firms are likely to be highly sensitive to prestige incentives, such as the shame of exclusion. Thus, under our prestige-incentives hypothesis, we would expect to observe a larger ROE response from Nikkei225 firms than from non-Nikkei225 firms.

We test the prestige-incentive hypothesis empirically in two ways. First, we employ our main DID specifications but split our treatment group indicator into *Nikkei225* and *non-Nikkei225* subgroups. Such a specification allows for treatment differences but assumes that there are no Nikkei225 and non-Nikkei225 specific effects; that is, it does not allow the controls to differ based on Nikkei225 status. This translates into estimating the following equation:

$$
\begin{aligned}
ROE_{i,t+1} = \; & \alpha + \beta_1 Treat_{i,t} \times Nikkei225_{i,t} \times Post_t \\
& + \beta_2 Treat_{i,t} \times non\text{-}Nikkei225_{i,t} \times Post_t + \beta_3 Treat_{i,t} \times Nikkei225_{i,t} \\
& + \beta_4 Treat_{i,t} \times non\text{-}Nikkei225_{i,t} + \gamma X_{i,t} + f_t + \epsilon_{i,t}. \qquad (4)
\end{aligned}
$$

In this estimation, we are interested in the sign and significance of the two triple interaction coefficients as well as testing for their equality. Under the prestige-incentive hypothesis, $beta_1$ should be larger in magnitude than and statistically different from $beta_2$.

To complement these tests, we compare the DID in ROE of the two subsets by employing a difference-in-difference-in-differences (DDD) design. This test is more general because it allows for Nikkei225 and non-Nikkei225 specific effects. More specifically, we estimate the following equation:

$$\begin{aligned} ROE_{i,t+1} &= \alpha + \beta_1 Treat_{i,t} \times Nikkei225_{i,t} \times Post_t + \beta_2 Nikkei225_{i,t} \times Post_t \\ &\quad + \beta_3 Treat_{i,t} \times Post_t + \beta_4 Nikkei225_{i,t} + \beta_5 Treat_{i,t} + \gamma X_{i,t} + f_t + \epsilon_{i,t}. \quad (5) \end{aligned}$$

In this estimation, we are interested in the sign and significance of the triple interaction coefficient. Under the prestige-incentive hypothesis, $beta_1$ should be positive and statistically significant.

In addition to the above tests, we also estimate complementary versions that use *Quintile(Closeness)*, the continuous treatment measure introduced in Table 4, in place of the binary treatment measure *Treat*. All other variables used in these empirical specifications are as described in the discussions of Tables 3 and 4.

In estimating these equations, we use the expanded data set spanning the the entire ranked set of 1,000 firms. For one, we do so because the *Quintile(Closeness)* treatment measure is defined on and designed to exploit the variation in treatment intensity across the full data. Moreover, the expanded sample helps to alleviate concerns about the precision with which we can estimate the parameters of (and the statistical power with which we can test the estimated parameters of) these triple-interaction models, in particular given the presence of Nikkei225 firms ranked 400 or above control group is relatively sparse.

Columns 1 and 2 of Table 5 report the results of estimating equation 4 using our binary treatment measure and the *Quintile(Closeness)* treatment measure, respectively. In column (1), the estimated coefficient of 0.066 on *Treat*×*Nikkei225*×*Post* is six times the estimated coefficient of 0.011 on *Treat*×*non-Nikkei225*×*Post* (a difference that is statistically significant at the 5% level), consistent with prestige incentives being the primary driver of the overall ROE effects. Similarly, in column 2, the estimated treatment response of Nikkei225 firms is larger in magnitude—over three times as large—and statistically different from that of non-Nikkei225 firms at the 5% level.

Columns 3 and 4 report coefficient estimates from equation 5. In column 3, using our binary treatment measure, we report a coefficient of 0.050 on the triple interaction, *Treat*×*Nikkei225*×*Post*; the DDD estimate is both economically and statistically significant. The point estimate indicates that the treatment response for the Nikkei225 firms is more than 5 percentage points higher than that of the non-Nikkei225 firms; moreover, these results suggest that the DID estimate for Nikkei225 firms is statistically different from the DID estimate for non-Nikkei225 firms at the 5% level. Column 4 repeats this exercise using the *Quintile(Closeness)* treatment measure. We find a positive and significant (at the 10% level) coefficient on the triple interaction, *Quintile(Closeness)*×*Nikkei225*×*Post*, further evidence of a stronger treatment response for Nikkei225 firms. Collectively, these results suggest that the dominant incentive channel driving the overall ROE effect is prestige rather than capital-market benefits associated with index inclusion.

4.4 Drivers of the ROE Response

Next we study which levers managers pull to generate the improvement in ROE that we document. Table 6 reports the results of regressions examining the behavior of the major drivers of ROE: return on assets, which is driven by profit margins and asset utilization, and financial leverage. In columns 1-4 of Panel A, we replicate our main baseline specification (column 4 of Table 3) but use *Forward ROA*, *Forward Profit Margin*, *Forward Asset Turnover*, and *Forward Leverage* respectively as dependent variables. In Panel B, in order to examine the cross-sectional variation in the types of channels that different firms may utilize to increase ROE, we run each specification of Panel A for the subset of companies with above-median and below-median contemporaneous values in the ROE driver of interest.

The results in Table 6 suggest that the improvement in ROE is largely driven by improving operations (increasing ROA) rather than by changing capital structure. Panel A, column 1, reports a statistically significant (at the 5% level) treatment effect on *Forward ROA* of 0.53 percentage points, a 17% increase over the treatment group's pre-period mean of 3.1%. Panel A, column 2, suggests that this increase in ROA is primarily driven by improved *Forward Profit Margin*: the statistically significant (at the 5% level) treatment effect estimate of 0.0074 constitutes a 15.3% increase in margins relative to their pre-period average of 4.9%. Panel B, columns 1 and 2, document that the increase in net margins is driven by the subsample of treatment firms with below-median margins: the coefficient on $Treat \times Post$ is significant only for the below-median profit-margin sample, and the point estimate of the treatment effect is 0.0158, more than twice the treatment effect for the full sample.

In contrast, our analysis of *Forward Asset Turnover* suggests that improvement in asset efficiency is not the dominant channel for improvement in ROA. The treatment effect for the full sample, reported in Panel A, column 3, is not statistically significant at the 10% level. However, the subsample analysis in Panel B, columns 3 and 4, reveals a statistically significant increase in utilization among those firms with below-median asset turnover. Nevertheless, this effect, at around 1.96% of the treatment group's average pre-period turnover, is economically much smaller than the treatment effect in profit margins.

Finally, we find no evidence that financial leverage was significantly impacted by firms' JPX400-inclusion incentives, either for the overall sample (Panel A, column 4) or for the subsamples of above-median and below-median-leverage firms (Panel B, columns 5 and 6). These findings are not entirely surprising, since JPX400's ranking algorithm does not necessarily incentivize firms to increase leverage per se. Under Modigliani and Miller (1958) assumptions, a profitable firm making a simple *ceteris paribus* increase in financial leverage—such as by issuing debt and repurchasing shares—would increase the company's ROE but might decrease its total market capitalization, a component of the selection algorithm.

On the other hand, it is possible that our measure of financial leverage (net debt over equity) is too noisy a measure to capture JPX400's potential effects on firm's capital-structure decisions. We thus complement the analysis of financial leverage by examining the effect of the JPX400-inclusion incentives on payout policy, using shareholder payout ratio (i.e., dividends plus repurchases divided by shareholder's equity) as the dependent variable of interest. Treated firms could increase shareholder payouts to boost their ROE, by reducing retained earnings and the book value of shareholders equity, particularly if doing so leads shareholders to increase valuation multiples (thus dampening the potential tradeoff in market capitalization). In this way we can interpret shareholder payouts as a financing choice that firms can employ to boost ROE, but where we are more likely to find a statistically measurable effect than in net debt leverage. This variable is also of interest given that, as (Ito, 2014) asserts, Japanese policy makers were interested in changing Japanese corporations cash-hoarding cultures.

In column 1 of Table 7, we find a statistically significant effect (at the 5% level) on shareholder payouts overall. Moreover, in columns 2 and 3 we show that that this effect is driven by the subsample of firms with more excess cash—that is, those with above-median cash-to-equity ratios. This overall effect size—0.59 percentage points—is economically large, representing approximately a 21% increase in payout ratio relative to the pre-period treatment group mean. Relative to total cash and investment on treatment firms' balance sheets, however, the magnitude of the payouts is quite small: our estimates suggest that shareholder payouts represent 1.5% of treatment-group pre-period cash to equity ratio. This might explain why an effect driven through the shareholder-payouts channel is statistically difficult to detect by examining financial leverage. Another reason is that, all else equal, JPX400's positive effects on earnings may have a countervailing and positive effect on total cash, lowering financial leverage.

Jointly, the analyses in Tables 6 and 7 show that JPX400-inclusion incentives drove firms to improve ROE through the channels where they had the most slack. We find that the overall effects are driven by operating changes—that is, by improving ROA. Nonetheless, we also find a significant increase in shareholder payouts.

4.5 Accruals, Investments, R&D, Employment, and Compensation

The results we have presented so far demonstrate that the JPX400 index drove a significant, robust improvement in ROE. An interesting question is whether the index-inclusion incentives may also have produced potentially undesirable effects. For example, managers could have improved accounting earnings by manipulating accruals or by cutting productive investments, contrary to the JPX400's broader goals of improving capital efficiency and social welfare in Japan.[12]

Table 8 explores these possibilities by estimating the incentive effects of JPX400 inclusion on six different outcome variables: *Forward Accruals to Assets*, *Forward Log NOA*, *Forward R&D to Sales*, *Forward Log Employees*, *Forward Log Average Employee Pay*, and *Forward Log Average Executive Pay*. Each regression in this table employs the main specification

[12] A long literature in accounting, economics, and finance (e.g., Stein, 1989; Healy, 1985) has documented that incentivizing managers using relatively short-term earnings measures can produce unintended, and potentially value-reducing, consequences.

from column 4 of Table 3: DID in the dependent variable of interest with firm-level controls and a control for the lagged dependent variable.

Column 1 reports the results of the specification with *Forward Accruals to Assets* as the dependent variable. We use income-statement accruals as a proxy for earnings management. The coefficient on $Treat \times Post$ is not statistically significantly different from zero, suggesting that the increase in ROE is not driven by accruals-based earnings management.

In order to examine the index-inclusion effects on "real" investments in net operating assets, column 2 reports the results of the specification with *Log NOA* as the dependent variable. Once again, we find no statistically significant treatment effect on *Log NOA*, suggesting that the JPX400 did not drive a statistically measurable change in investments in net operating assets.

Column 3 examines how firms alter R&D intensity (R&D-expense-to-sales ratio) in response to the JPX400-inclusion incentives. R&D is often seen as having positive externalities and long-term benefits that are not captured by the accounting system, but—as a discretionary expense item—managers might cut R&D in order to boost reported earnings (e.g., Roychowdhury, 2006). R&D expenditures could thus potentially entail a tradeoff between ROE and the macroeconomic and social-welfare goals of Japanese policy makers. The point estimate on $Treat \times Post$ in column 3 of -.0011 is significant at the 10% level and indicates a 7.05% reduction in R&D intensity relative to the treatment group's pre-period mean of 1.56%. This finding is consistent with Table 6, which shows that the improvement in ROE is overall driven by improving profit margin. Our findings here suggest that managers did so at least in part by cutting discretionary expenses like R&D.

Next, we examine how firm-level employment responds to the incentives generated by the JPX400. Japanese firms are well known for having historically had an implicit system of lifetime employment. This system has been weakened in recent years, but Japanese firms are still more reluctant than Western firms to downsize employment. Some policy makers view these employment norms as a barrier to dynamism and growth; others see them as providing social-welfare benefits to Japanese workers. Either way, any employment effects attributable to JPX400-inclusion incentives are policy relevant.

Column 4 reports the result of the main DID specification with *Forward Log Employees* as the dependent variable. We find no evidence of statistically or economically significant changes in firm-level employment, suggesting that our main ROE result is unlikely to be driven by employee downsizing.

Finally, we examine the impact of the index on average employee pay (column 5) and average pay of executive officers and directors (column 6). The coefficients on $Treat \times Post$ in both columns are insignificant, meaning that we find no evidence of an increase in compensation in treated firms. The lack of significance on executive pay could be seen as surprising given the large increases in ROE for treated firms documented above. However, this finding is consistent with a low level of pay-performance sensitivity in Japan that high-powered incentives are not the norm. This further bolsters our interpretation that the pursuit of prestige, rather than the incentive effects from compensation or formal contracts, is what drove the managers of treated firms to improve performance.

Collectively, the results in Table 8 suggest that the main ROE effect we document is not driven by significant earnings management or significant cuts to capital investments in operating assets, employment, or employee compensation. However, the ROE effects are driven at least in part by cutting discretionary expenses such as R&D.

4.6 Market Valuation

Our final analysis focuses on market-valuation outcomes associated with JPX400-inclusion incentives. Table 9 reports the results of our main DID specification using *Forward Book to Market* ratio as the outcome variable of interest. Column 1 reports the results of a reduced-form OLS DID with firm-level controls and time fixed effects. The negative, statistically significant coefficient on $Treat \times Post$ in this column indicates that treatment firms experienced a relative improvement in their valuations multiples. The point estimate of -.035 represents a 3.4% decrease in book-to-market relative to the pre-period treatment-group mean of 1.023. This finding suggests that the improvement in ROE for treatment firms led to an upward revision in the market's expectations about their future cash flows and that this revision was not compensated for by a commensurate increase in expectations of firm risk.

Columns 2 and 3 supplement this analysis by implementing a two-stage least-squares estimation of the effect of *Forward ROE* on *Forward Book to Market*, using the DID interaction as the instrument for *Forward ROE*. Column 2 reports the results of the first stage of the estimation, which is similar to our main result in Table 3. Column 3 reports the results of the second stage, which in effect regresses *Forward Book to Market* on the predicted values of *Forward ROE*.[13] The negative, statistically significant coefficient on *Forward ROE* indicates that a 1-percentage-point increase in ROE due to the JPX400-inclusion incentives is estimated to yield a decrease in *Forward Log Book to Market* of -1.612. In other words, a 2.4-percentage-point increase in *Forward ROE*—our most robust DID estimates in Table 3—is expected to yield a decrease in *Forward Book to Market* of 0.0387, which is a 3.8% de-

[13] These estimates are obtained in one step; thus the second-stage standard errors account for the first-stage estimation noise.

crease relative to the pre-period treatment-firm mean and consistent with the reduced-form estimates in column 1. Overall, our evidence suggests that firms enjoyed an expansion in multiples as a result of their on-average increase in ROE due to the JPX400.

5 Discussion of the JPX400's Overall Effect

The JPX400 has generated significant interest. Among regulators, policy makers, and investors in Japan and globally, there is a lively discussion of the efficacy of this index. We conclude our analysis by assessing the overall impact of the index.

In our view, comparing the realized returns of the JPX400 to those of other Japanese indices, as some policy makers and commentators do, does not reflect the success of the index, nor does it appropriately capture the incentive effects of the index on corporate performance and value. Such an approach has two conceptual flaws. First, as our analyses show, part of the effect of the index resides in the performance effects on firms excluded from the index that are improving their performance to earn membership. Second, to the extent that valuation benefits accrue to such firms prior to their inclusion in the JPX400—due to market participants either responding to or anticipating firms' performance improvements resulting from JPX400-inclusion incentives—they would not be captured in the returns of the JPX400 index. Instead, the relative-return comparison would attribute the beneficial effect of the JPX400 to the benchmark index. These two factors could explain why the JPX400 has *under*performed the Nikkei225 over the three-year period from June 2014 to June 2017, and why some stocks outperform after being excluded from the JPX400.

Our research design avoids both of these conceptual issues by leveraging differences in the intensity of JPX400-inclusion incentives and by focusing on a fundamental measure of performance (ROE). Using our revised approach, we find that the JPX400 index has been substantially more successful than its portfolio returns would suggest.

To provide a conservative estimate of the JPX400's overall impact on the aggregate Japanese economy, we focus on the incremental income generated by the firms that we classify as "treated" due to their index-inclusion incentives. In untabulated results, we estimate a simple DID in forward net income using our baseline treatment and control groups, and find an on-average annual firm-level improvement in net income of JPY6.0 billion. Aggregating across the 200 firms in the treatment group yields a JPX400 effect on aggregate net income of JPY1.2 trillion per year. This represents an 8.9% increase relative to pre-period average aggregate income across all public Japanese firms (JPY13.6 trillion per year). Moreover, relative to the change in average aggregate net income from the pre-period to the post-period (from JPY13.6 trillion to JPY21 trillion per year), the effects attributable to the JPX400 account for 16%.[14]

To estimate the total wealth or valuation effects, we multiply the JPX400's net-income effect by a range of plausible incremental price-to-earnings (P/E) multiples. A very conservative and lower-bound incremental P/E ratio on the new profits generated by the JPX400 would be 1: this would be the appropriate incremental P/E on the new profits if the markets perceived the earnings boost as completely unsustainable, and not likely to generate any internal alpha if reinvested. Under this assumption, the wealth effect would be JPY1.2 trillion per year, for a total of JPY3.6 trillion, or a 0.77% increase relative to the 2014 total Japanese market capitalization of JPY469 trillion. A less conservative incremental P/E ratio would be the treatment firms' cash-adjusted P/E ratio: $(MCap - Cash)/NetIncome$.[15] The average

[14]Computations in this section uses the Datastream universe of public Japanese firms in computing aggregate earnings and market capitalization.

[15]This multiple is justified under the assumptions that (a) the market values Japanese corporations' cash holdings 1-for-1, such that the cash-adjusted P/E ratio captures the market's valuation of the companies'

cash-adjusted P/E multiple for treated firms in the post period is 17.07, implying a valuation impact of JPY61.45 trillion, or approximately 13.10% of total market capitalization in June 2014. Taking the midpoint of these P/E multiple estimates, our back-of-the-envelope estimation suggests that JPX400 improved the overall Japanese market's market capitalization by roughly 6.9%. Compared to the growth in market capitalization (measured as of end-June) between 2014 and 2017 of JPY163 trillion, these estimates suggest that the JPX400 explains approximately 20% of the overall market capitalization growth since the introduction of the index.

These estimates may also be conservative to the extent that firms further from the threshold are still affected by JPX400-inclusion incentives, though to a lesser degree than firms nearer the threshold of inclusion. We emphasize, however, that these back-of-the-envelope estimates are meant to characterize the approximate magnitude of JPX400's effect on Japanese corporate profitability and market capitalization. At face value, these estimates would suggest that the JPX400 had a meaningful impact on aggregate corporate profitability and valuation.

6 Conclusion

We provide evidence that prestige—aspiration to acquire prestige and shame at loss of prestige—can act as a powerful influence on how organizations and managers behave. We also show that stock indexes can be actively employed as sources of prestige and thus can influence managerial and corporate behavior. In the case of Japan, where there are limits to contracting, our evidence suggests that a prestige index helped to resolve a longstanding

earnings; and (b) the market perceives the income effect as sustainable, so that expected growth of and discount rates on the incremental profitability remain fixed.

problem of low corporate capital productivity. Our evidence suggests that, for firms near the threshold of inclusion in the JPX400, prestige incentives produced economically large and statistically significant increases in ROE.

This paper provides evidence that prestige-based incentives can be powerful in transforming corporate-governance norms in settings like Japan. These findings can inform corporate-governance reform efforts in capital markets with similar patterns of low capital efficiency and weak de-facto shareholder rights, such as those of other East Asian economies like China, Korea, Singapore, and Taiwan. We stress, however, that our evidence do not necessarily speak to the sustainability of these incentive effects.

Our findings are also important for developed markets. Given the growing calls for limiting executive compensation in the United States and in Europe, understanding non-pecuniary mechanisms for eliciting meaningful changes in corporate behavior is useful in the evolving corporate governance arena.

Indeed, our findings are timely in informing the apparent growing interest in, and debate about, using stock indexes to transform corporate norms across the world. In July 2017, two of the largest index providers—S&P Dow Jones in the U.S. and FTSE Russell in the U.K.—announced their decisions to exclude certain companies with multiple-voting class structures from their indexes. In March 2017, TSE and Nikkei announced a new index, the JPX-Nikkei Mid and Small Cap Index, extending profitability-based incentives to mid- and small-cap TSE-traded firms; similarly, MSCI announced the Japan Empowering Women index, backed by the GPIF and designed to shift Japanese norms around gender diversity in the workplace. Globally, there is a growing number of ESG-based indexes. Our findings highlight managers' concerns for prestige as an important channel through which indexes can motivate and influence changes in corporate behavior. We look forward to further research on the role of prestige incentives and the role of stock indexes in transforming corporate behavior.

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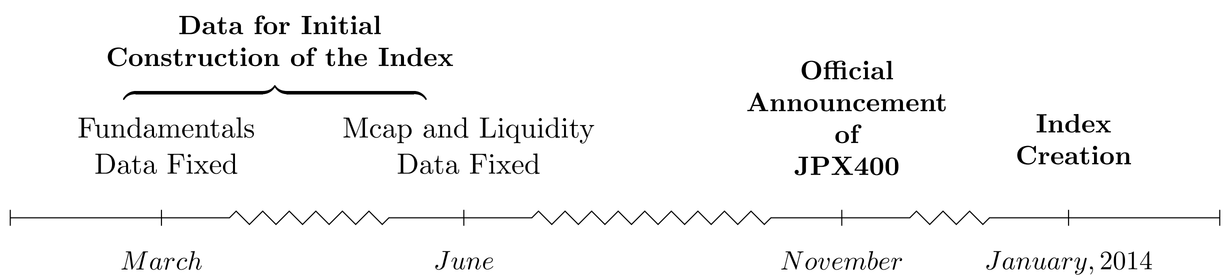
Fig. 1.

JPX400 Index Constituent Selection Time Line

Figure 1 is a schematic representation of the timeline of index selection, relative to firm-level information, for each year of the JPX400's existence. Panel A is a timeline of the initial construction of the index in 2013; Panel B is a timeline of the annual index rebalancing from 2014 onward. Vertical lines indicate important dates.

A: 2013



B: 2014+

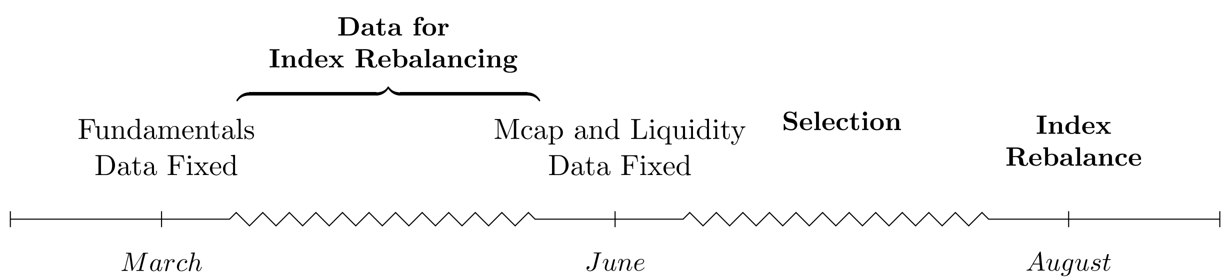


Fig. 2.

Sample Construction

Figure 2 is a schematic representation of the process we follow to construct our baseline analysis sample. Steps 1 and 2 mimic the JPX400 selection algorithm. At the end of June each year, we select the 1,200 most liquid firms trading in Japan and then winnow down this number to the 1,000 largest firms in terms of market capitalization. Thereafter we follow the JPX400 ranking algorithm and compute a synthetic JPX400 rank for each of the 1,000 firms. Step 3 describes how we assign treatment status based on this synthetic JPX400 ranking. The top 400 firms are our predicted JPX400 constituents. For our baseline analysis, we classify firms with ranks of 301–500 as the treatment group, and firms with ranks of 501–800 as the control group. We do not include firms with ranks of 1–300 or 801–1,000 in our baseline analysis, but include them in robustness tests.

Step 1: Each June, select top 1,000 firms by market capitalization and liquidity.

Step 2: Predict JPX400 rank using fundamental data available as of March and .

Step 3: Construct baseline treatment and control sample.

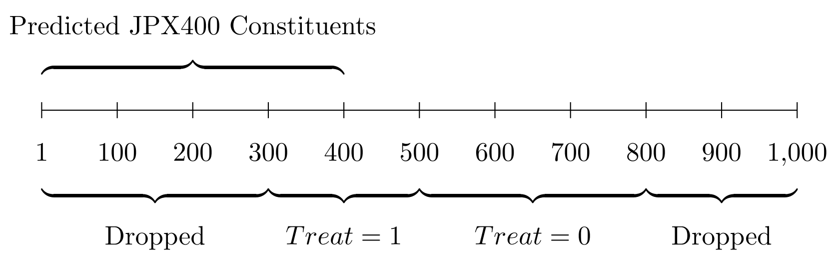


Fig. 3. Placebo Tests by Time

Figure 3 displays the results of placebo versions of our main analysis. We use historical data going back to 1991 to synthetically reconstruct JPX400 ranks and implement our main difference-in-differences test for six sets of years prior to the launch of the JPX400 in 2013. We mimic the temporal structure of our main analyses, and include seven years for each placebo test: three pre-treatment years and three post-treatment years; the treatment year is excluded. The figure reports the point estimate and the 95% confidence intervals for six placebo tests. The y-axis reports the magnitudes of the estimated treatment effect; the x-axis reports the mid-point or the dropout year of the seven-year window around which the treatment effect is estimated. The first placebo test uses data from 1994–2000, with 1994–1996 as the pre-placebo-treatment period, 1998–1999 as post-placebo-treatment, and 1997 as the midpoint and dropout year. The remaining placebo tests roll forward the window of examination by three years so that the 3-year pre-placebo-treatment windows are non-overlapping.

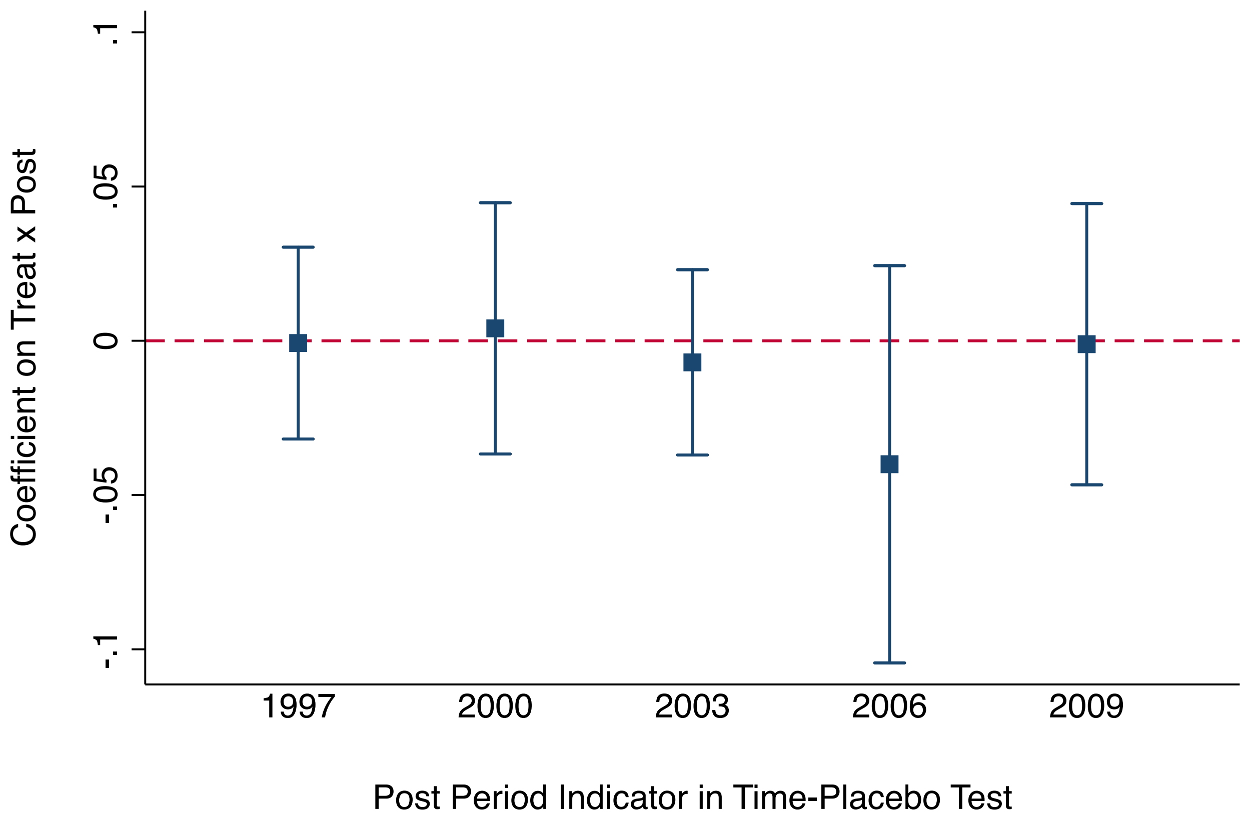


Table A1.

Description of Variables

This table presents definitions of variables used in our analysis. Nikkei membership data is taken from https://indexes.nikkei.co.jp/en/nkave/index/component. Average employee compensation and average executive officers and directors compensation are from a proprietary dataset collected by Toyo Kezai. All other data are obtained from the Thomson Reuters Datastream database, and their variable names are referred to in brackets in the computation column.

Variable	Description	Computation
Dependent Variables		
Asset Turnover	Asset turnover ratio	Revenues [WC07240] / Total Assets [WC02999]
Book to Market	Book-to-market ratio	Total Shareholders' Equity [WC03995] / Market Value [MV]
Dividends to Equity	Dividends to equity ratio	Cash Dividends Paid [WC04551] / Total Shareholders' Equity [WC03995]
Leverage	Financial leverage	(Long Term Debt [WC03251] - Cash & Short-Term Investments [WC02001]) / Total Shareholders' Equity [WC03995]
Log Avg EE Pay	Natural logarithm of the average employee pay	
Log Avg Exec Pay	Natural logarithm of the average pay for executives and directors	
Log Employees	Natural logarithm of the number of employees	ln(Employees [WC07011])
Profit Margin	Net profit margin	Net income [WC07250] / Revenues [WC07240]
Repurchases	Estimated repurchases	Funds to Decrease Common or Preferred Stock [WC04751] – Change in Preferred Stock [WC03451]
ROE	Return on equity	Net Income [WC07250] / Total Shareholders' Equity [WC03995]
ROA	Return on assets	Net Income [WC07250] / Total Assets [WC02999]
Shareholder Payouts	Shareholder payout ratio	(Repurchases + Cash Dividends Paid) / Total Shareholders' Equity [WC03995]

Table A1.
(Continued)

Control Variables		
Log Book to Market	Natural logarithm of book-to-market ratio	ln(Total Shareholders' Equity [WC03995] / Market Value [MV])
Accruals to Assets	Ratio of total accruals to total assets	(Net Income [WC07250] – Funds from Operations [WC04201]) / Total Assets [WC02999]
Log NOA	Natural logarithm of net operating assets	ln(Long-Term Debt [WC03251] − Cash & Short-Term Investments [WC02001] + Total Shareholders' Equity [WC03995])
R&D to Sales	R&D intensity	R&D Expenses [WC01201] / Revenues [WC07240]
Cash to Equity	Ratio of cash to total equity	Cash & Short-Term Investments [WC02001] / Total Shareholders' Equity [WC03995]
Log Market Cap	Natural logarithm of market capitalization	ln(Market Value [MV])
LT Debt to Equity	LT debt leverage	Long-Term Debt [WC03251] / Total Shareholders' Equity [WC03995]
Sales Growth	Sales growth	Revenues [WC07240] / Lagged Revenues [WC07240]
Nikkei225	Nikkei225 indicator	Indicator for a company's inclusion in the Nikkei225 index at the time of the JPX400 selection in the given year

Table 1.
Predicting JPX400 Membership

Columns 1 and 2 of this table reports estimates from OLS regressions of *Actual Inclusion*—an indicator for whether a firm is selected for the JPX-Nikkei400—on *Predicted Inclusion*—an indicator for whether a firm's synthetic rank falls between 1 and 400. In columns 3 and 4, *Predicted Inclusion* is disaggregated into two indicators, which specify whether a firm's synthetic rank falls within 1–200 or within 201–400. Three other indicators—for ranks within 400–600, within 600–800, and over 800—are also included in these regressions. These indicators subsume the constant term in the regression. Columns 2 and 4 include other specified firm-level controls. The sample consists of firms with synthetic ranks of 1–2000 in the years 2013–2016. Standard errors, clustered at the firm-level, are reported in parentheses. Significance levels are indicated by *, **, *** for 10%, 5%, and 1% respectively.

	(1) *Actual Inclusion*	(2) *Actual Inclusion*	(3) *Actual Inclusion*	(4) *Actual Inclusion*
Predicted Inclusion	0.923***	0.864***		
	(0.01)	(0.01)		
Rank 1–200			0.991***	0.949***
			(0.00)	(0.01)
Rank 201 – 400			0.879***	0.857***
			(0.01)	(0.02)
Rank 401 – 600			0.0542***	0.0457***
			(0.01)	(0.01)
Rank 601 – 800			0	0
			(.)	(.)
Rank outside 800			0.00488***	0.0315***
			(0.00)	(0.01)
Log Market Cap		0.0217***		0.0185***
		(0.00)		(0.00)
Log Book to Market		-0.00180		-0.000153
		(0.00)		(0.00)
Sales Growth		-0.000275		0.00115
		(0.01)		(0.01)
LT Debt to Equity		-0.00120		-0.000453
		(0.00)		(0.00)
Cash to Equity		0.00134		-0.0000954
		(0.00)		(0.00)
Observations	7,110	7,110	7,110	7,110
R^2	0.869	0.872	0.900	0.876

Table 2.

Summary Statistics

Table 2 reports summary statistics on the pre-treatment period (2010–2012) for the full sample used for the main regressions (firms synthetically ranked 301–800). The rightmost three columns report the means for the treatment group (ranks 301–500) and the control group (ranks 501–800) and T-statistic from the test of equality in means between the two groups. All continuous variables are winsorized at the top and bottom 0.5% of the cross-sectional distribution.

	Full Pre-Period Sample							
	p25	*Mean*	*p50*	*p75*	*SD*	*Mean* $Treat = 1$	*Mean* $Treat = 0$	*T-Stat of Difference*
ROE	0.0300	0.0541	0.0500	0.0821	0.1007	0.0585	0.0512	1.3145
ROA	0.0086	0.0284	0.0238	0.0427	0.0382	0.0311	0.0267	2.1998
Profit Margin	0.0136	0.0469	0.0301	0.0605	0.0855	0.0485	0.0459	0.5784
Asset Turnover	0.6064	0.9565	0.8990	1.2423	0.6316	1.0121	0.9197	2.7198
Leverage	1.5852	3.7139	2.0729	3.2311	4.7584	3.7655	3.6798	0.3488
Repurchases to Equity	0.0000	0.0062	0.0000	0.0002	0.0270	0.0065	0.0060	0.3315
Dividends to Equity	0.0115	0.0205	0.0170	0.0241	0.0150	0.0213	0.0199	1.8276
Log Book to Market	-0.1845	0.0833	0.1559	0.4247	0.5348	0.0174	0.1269	-3.9403
Lagged Annual Return	-0.1701	-0.0389	-0.0092	0.1346	0.2826	-0.0409	-0.0376	-0.2256
Accruals to Assets	-0.0646	-0.0435	-0.0427	-0.0195	0.0383	-0.0453	-0.0423	-1.5218
Log NOA	17.2469	18.0358	18.0206	18.8100	1.3170	18.3899	17.7994	7.9311
R&D to Sales	0.0000	0.0172	0.0052	0.0254	0.0281	0.0156	0.0182	-1.8103
Log Employees	7.3702	8.1015	8.0467	8.7921	1.2670	8.4527	7.8655	8.6375
Log Market Cap	17.4785	18.0954	17.9597	18.6252	0.8692	18.5053	17.8244	15.3226
Sales Growth	0.9365	1.0127	1.0041	1.0741	0.1711	1.0093	1.0151	-0.6626
LT Debt to Equity	0.0172	0.4040	0.1600	0.4920	0.6759	0.4632	0.3649	2.7037
Cash to Equity	0.1868	0.3918	0.3036	0.4411	0.4078	0.4005	0.3861	0.6322
Nikkei 225	0.0000	0.1440	0.0000	0.0000	0.3512	0.2060	0.1030	5.3076

Table 3.

Treatment Effects in ROE

Table 3 reports the estimates of DID regressions using *Forward ROE* as the dependent variable. *Treat*, an indicator denoting the treatment group, is equal to one for firms ranked 301–500 under the our replication of the JPX400 selection algorithm and zero for firms ranked 501–800; *Post* is an indicator variable assuming a value one for the years 2014–2016 and zero for the pre-treatment period, 2010–2012. Column 1 reports a baseline specification with no controls or fixed effects; column two adds year-fixed effects; and column 3 adds industry-fixed effects, where industry is defined by Datastream's Industry Level 3 Name (INDM3), and firm-level controls. Column 4 reports results using firms' contemporaneous ROE as a control, as well as specified controls and time fixed effects. Column 5 examines adds two additional interaction terms—*Treat* x *(Year = 2011)* and *Treat* x *(Year = 2012)*—to the specification of column 4 to test differences in pre-treatment trends. All firm-level control variables are defined in Table A1. Standard errors, clustered at the firm level, are reported in parentheses. Significance levels are indicated by *, **, *** for 10%, 5%, and 1% respectively.

	(1) *Forward ROE*	(2) *Forward ROE*	(3) *Forward ROE*	(4) *Forward ROE*	(5) *Forward ROE*
Treat x *Post*	0.028***	0.028***	0.025***	0.024***	0.022**
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Treat	-0.006	-0.006	0.007	-0.005	-0.003
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Post	0.018***				
	(0.01)				
Treat x *(Year = 2011)*					-0.005
					(0.01)
Treat x *(Year = 2012)*					-0.000
					(0.01)
ROE				0.384**	0.384**
				(0.15)	(0.15)
Log Market Cap			-0.026***	-0.013**	-0.013**
			(0.00)	(0.01)	(0.01)
Log Book to Market			-0.069***	-0.045***	-0.045***
			(0.01)	(0.01)	(0.01)
Sales Growth			0.038**	-0.011	-0.011
			(0.02)	(0.02)	(0.02)
LT Debt to Equity			-0.013*	-0.007	-0.007
			(0.01)	(0.00)	(0.00)
Cash to Equity			-0.016	-0.001	-0.001
			(0.01)	(0.01)	(0.01)
Time FE	No	Yes	Yes	Yes	Yes
Industry FE	No	No	Yes	No	No
Observations	2,783	2,783	2,783	2,783	2,783
R^2	0.0221	0.0219	0.2472	0.3031	0.3026

Table 4.
JPX400 Index-Inclusion Incentives

Table 4 reports the regression results of various modifications to the main DID specification in column 4 of Table 3. Each column reports a specification that considers different treatment and control definitions. Column 1 partitions *Treat* into two separate treatment groups —firms ranked 301–400 (those that barely make the JPX400 cutoff) and firms ranked 401–500 (those that barely miss the cutoff). The F-statistic, and its corresponding p-value, from a test of equality between the two partitioned DID coefficients are reported in the last two rows. Column 2 estimates a DID regression with an expanded sample that includes firms ranked 1–300 and estimates a separate treatment effect for these firms (the coefficient on *Rank 1-300* x *Post*). The F-statistic, and its corresponding p-value, from a test of equality between the two interaction terms are reported in the last two rows. Column 3 estimates the main DID specification using a sample of firms ranked 1301–1800, where the treatment firms are those ranked 1301–1500 and control firms are those ranked 1501–1800. Column 4 expands the column 2 sample to include the top 1000 firms; here, however, our treatment variable of interest is *Quintile(Closeness)*, a variable indicating the particular quintile in which a firm falls in terms of its closeness to the rank of 400. All firm-level control variables are defined in Table A1. Standard errors, clustered at the firm level, are reported in parentheses. Significance levels are indicated by *, **, *** for 10%, 5%, and 1% respectively.

	(1) *Forward ROE*	(2) *Forward ROE*	(3) *Forward ROE*	(4) *Forward ROE*
Treat x *Rank 301-400* x *Post*	0.027***			
	(0.01)			
Treat x *Rank 401-500* x *Post*	0.024**			
	(0.01)			
Treat x *Post*		0.023***		
		(0.01)		
Rank 1-300 x *Post*		0.006		
		(0.01)		
Placebo Group Treat x *Post*			0.002	
			(0.01)	
Quintile(Closeness) x *Post*				0.006**
				(0.00)
Treat x *Rank 301-400*	0.000			
	(0.01)			
Treat x *Rank 401-500*	-0.008			
	(0.01)			
Treat		-0.009		
		(0.01)		
Rank 1-300		0.014		
		(0.01)		
Placebo Group Treat			0.020***	
			(0.01)	
Quintile(Closeness)				-0.000
				(0.00)
ROE	0.379**	0.439***	0.171**	0.373***
	(0.15)	(0.12)	(0.07)	(0.05)
Time FE	Yes	Yes	Yes	Yes
Firm Controls	Yes	Yes	Yes	Yes
Observations	2,783	4,462	2,885	5,546
R^2	0.3040	0.3532	0.1196	0.2630
F-stat	0.059	7.719		
p-value	0.808	0.006		

Table 5.

Prestige vs. Other Index-Inclusion Incentives

Table 5 reports the OLS results from estimation of equations 4 (columns 1 and 2) and 5 (columns 3 and 4), using the full sample of firms ranked 1–1,000. In all columns, *Nikkei225* is indicates contemporaneous membership in the Nikkei 225 index; *Treat* is an indicator variable assuming a value of one for firms ranked 301–500 under the our replication of the JPX400 selection algorithm and zero for firms ranked 501–800; *Quintile(Closeness)* is a quintile ranking, ranging from the values of 0 to 4, of a firm's distance from the rank of 400, where the highest (lowest) quintile reflect those firms whose ranks are closest to (farthest from) 400; and *Post* is an indicator variable assuming a value one for the years 2014–2016 and zero for the pre-treatment period, 2010–2012. For columns 1 and 2,the last two rows report the F-statistic and its p-value from a Wald test of equality between the two triple-interaction coefficients. For all columns, we include time fixed effects and firm-level controls as in column 4 of Table 3, but coefficient estimates are unreported. All firm-level control variables are defined in Table A1. Standard errors, clustered at the firm level, are reported in parentheses. Significance levels are indicated by *, **, *** for 10%, 5%, and 1% respectively.

	Splitting Treatment		Triple Diffs	
	(1)	(2)	(3)	(4)
Treat x *Nikkei225* x *Post*	0.066***		0.050**	
	(0.02)		(0.02)	
Treat x *non-Nikkei225* x *Post*	0.011**			
	(0.01)			
Quintile(Closeness) x *Nikkei225* x *Post*		0.013***		0.015*
		(0.00)		(0.01)
Quintile(Closeness) x *non-Nikkei225* x *Post*		0.004		
		(0.00)		
Quintile(Closeness) x *Post*				0.003
				(0.00)
Nikkei225 x *Post*			0.006	-0.018
			(0.01)	(0.02)
Treat x *Post*			0.012**	
			(0.01)	
Treat x *Nikkei225*	-0.059***		-0.055**	
	(0.02)		(0.02)	
Treat x *non-Nikkei225*	0.003			
	(0.00)			
Quintile(Closeness) x *Nikkei225*		-0.009**		-0.017***
		(0.00)		(0.01)
Quintile(Closeness) x *non-Nikkei225*		0.002		
		(0.00)		
Quintile(Closeness)				0.003
				(0.00)
Nikkei225			-0.007	0.019
			(0.01)	(0.01)
Treat			0.001	
			(0.00)	
ROE	0.373***	0.367***	0.373***	0.366***
	(0.05)	(0.05)	(0.05)	(0.05)
Time FE	Yes	Yes	Yes	Yes
Firm Controls	Yes	Yes	Yes	Yes
Observations	5,546	5,546	5,546	5,546
R^2	0.2657	0.2661	0.2656	0.2663
F-stat	6.315	6.531		
p-value	0.012	0.011		

Table 6.

Drivers of ROE

Table 6 reports the results of DID regressions following the specification in column 4 of Table 3, but using as dependent variables the drivers of ROE —*Forward ROA*, *Forward Profit Margins*, *Forward Asset Turnover*, and *Forward Leverage*. As in Table 3, *Treat*, an indicator denoting the treatment group, is equal to one for firms ranked 301–500 under the our replication of the JPX400 selection algorithm and zero for firms ranked 501–800; *Post* is an indicator variable assuming a value one for the years 2014–2016 and zero for the pre-treatment period, 2010–2012. *Panel A* reports the results estimated on the full sample; *Panel B* reports results estimated on subsamples. The starting sample for all analyses is one for which all variables used in the baseline analysis—columns 3-5 of Table 3—are available. All firm-level control variables are defined in Table A1. Standard errors, clustered at the firm level, are reported in parentheses. Significance levels are indicated by *, **, *** for 10%, 5%, and 1% respectively.

Panel A: Full Sample Analysis

	(1) *Forward ROA*	(2) *Forward Profit Margins*	(3) *Forward Asset Turnover*	(4) *Forward Leverage*
Treat x *Post*	0.0053**	0.0074**	0.0047	-0.0048
	(0.002)	(0.004)	(0.008)	(0.016)
Treat	-0.0020	-0.0147***	-0.0161***	0.0077
	(0.002)	(0.003)	(0.006)	(0.013)
ROA	0.5307***			
	(0.058)			
Profit Margin		0.8153***		
		(0.063)		
Asset Turnover			0.9564***	
			(0.007)	
Log Market Cap	-0.0026**	0.0058***	0.0058**	0.0173**
	(0.001)	(0.001)	(0.003)	(0.007)
Log Book to Market	-0.0201***	-0.0175***	-0.0035	0.0331***
	(0.003)	(0.005)	(0.006)	(0.012)
Sales Growth	-0.0167**	-0.0402***	-0.0109	0.0728**
	(0.007)	(0.010)	(0.026)	(0.034)
LT Debt to Equity	-0.0052***	0.0026	0.0006	0.9456***
	(0.001)	(0.002)	(0.003)	(0.021)
Cash to Equity	-0.0047**	-0.0013	-0.0090	-0.8277***
	(0.002)	(0.004)	(0.007)	(0.032)
Time FE	Yes	Yes	Yes	Yes
Industry FE	No	No	No	No
Observations	2,784	2,785	2,784	2,783
R^2	0.5002	0.6261	0.9538	0.8875

Table 6. Continued

Panel B: Subsample Analysis

	Forward Profit Margin		Forward Asset Turnover		Forward Net Debt Leverage	
	(1) Below-Median Profit Margin	(2) Above-Median Profit Margin	(3) Below-Median Asset Turn	(4) Above-Median Asset Turn	(5) Below-Median Leverage	(6) Above-Median Leverage
Treat x *Post*	0.0158***	0.0010	0.0180***	-0.0104	0.0254	-0.0213
	(0.006)	(0.005)	(0.007)	(0.015)	(0.020)	(0.026)
Treat	-0.0090*	-0.0104**	-0.0249***	-0.0078	-0.0142	0.0242
	(0.005)	(0.004)	(0.005)	(0.010)	(0.014)	(0.021)
Profit Margin	0.0910	0.9318***				
	(0.112)	(0.025)				
Asset Turnover			0.9919***	0.9442***		
			(0.006)	(0.011)		
Log Market Cap	-0.0038**	0.0096***	0.0069***	0.0044	0.0063	0.0234**
	(0.002)	(0.002)	(0.002)	(0.005)	(0.007)	(0.011)
Log Book to Market	-0.0148***	-0.0113***	-0.0192***	0.0062	0.0232**	0.0518**
	(0.004)	(0.004)	(0.004)	(0.011)	(0.011)	(0.024)
Sales Growth	-0.0067	-0.0344**	-0.0239	0.0083	0.0733**	0.0667
	(0.013)	(0.015)	(0.024)	(0.049)	(0.037)	(0.061)
LT Debt to Equity	-0.0051**	-0.0052	0.0012	0.0213	0.6923***	0.9515***
	(0.002)	(0.006)	(0.002)	(0.014)	(0.105)	(0.029)
Cash to Equity	0.0060	-0.0082*	-0.0003	-0.0291	-0.7505***	-0.8910***
	(0.006)	(0.005)	(0.004)	(0.022)	(0.049)	(0.061)
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	No	No	No	No	No	No
Observations	1,255	1,530	1,355	1,429	1,281	1,502
R^2	0.0700	0.7175	0.9320	0.9036	0.7293	0.8546

Table 7.
Shareholder Payouts

Table 7 reports the results of DID regressions following the specification in column 4 of Table 3, but using *Shareholder Payouts* as the dependent variable. As in Table 3, *Treat*, an indicator denoting the treatment group, is equal to one for firms ranked 301–500 under the our replication of the JPX400 selection algorithm and zero for firms ranked 501–800; *Post* is an indicator variable assuming a value one for the years 2014–2016 and zero for the pre-treatment period, 2010–2012. Column 1 reports results for the regression estimated on the full sample; columns 2 and 3 report results estimated on the subsample of firms firms with above-median and below-median contemporaneous cash-to-equity ratios. The starting sample for all analyses is one for which all variables used in the baseline analysis–columns 3-5 of Table 3—are available. All firm-level control variables are defined in Table A1. Standard errors, clustered at the firm level, are reported in parentheses. Significance levels are indicated by *, **, *** for 10%, 5%, and 1% respectively.

	(1) All	(2) Below-Median Cash-to-Equity	(3) Above-Median Cash-to-Equity
Treat x *Post*	0.0059**	0.0023	0.0095*
	(0.003)	(0.002)	(0.005)
Treat	-0.0039***	-0.0011	-0.0083***
	(0.001)	(0.001)	(0.002)
Repurchases to Equity	0.0147	0.1370*	-0.0315
	(0.033)	(0.083)	(0.028)
Dividends to Equity	0.9491***	0.8850***	0.9872***
	(0.070)	(0.057)	(0.105)
Log Market Cap	-0.0001	0.0004	0.0009
	(0.001)	(0.001)	(0.001)
Log Book to Market	-0.0077***	-0.0055***	-0.0081***
	(0.002)	(0.001)	(0.003)
Sales Growth	0.0071	0.0003	0.0127
	(0.007)	(0.004)	(0.011)
LT Debt to Equity	-0.0010	-0.0027***	0.0001
	(0.001)	(0.001)	(0.002)
Cash to Equity	0.0051	-0.0037	0.0025
	(0.003)	(0.006)	(0.005)
Time FE	Yes	Yes	Yes
Industry FE	No	No	No
Observations	2,781	1,548	1,233
R^2	0.2611	0.3026	0.2449

Table 8.

Accruals and Investments

Table 8 reports the results of DID regressions following the specification in column 4 of Table 3, but using alternative outcome variables: *Forward Accruals to Assets*, *Forward Net Operating Assets*, *Forward R&D to Sales*, *Forward Log Employees*, *Forward Log Average Employee Pay*, and *Forward Log Average Executive Pay*. As in Table 3, *Treat* is equal to one for firms ranked 301–500 under the JPX400 selection algorithm and zero for firms ranked 501–800; *Post* is an indicator variable assuming a value of one for years 2014–2016 and zero for the pre-treatment period, 2010–2012. The starting sample for all analyses is one for which all variables used in the baseline analysis—columns 3-5 of Table 3—are available. Observations vary across specifications depending on the availability of specific new variables used for the analysis. All firm-level control variables are defined in Table A1. Standard errors, clustered at the firm level, are reported in parentheses. Significance levels are indicated by *, **, *** for 10%, 5%, and 1% respectively.

	(1) *Forward Accruals to Assets*	(2) *Forward Log NOA*	(3) *Forward R&D to Sales*	(4) *Forward Log Employees*	(5) *Forward Log Avg EE Pay*	(6) *Forward Log Avg Exec Pay*
Treat x *Post*	0.0017	0.0329	-0.0011*	0.0050	0.0045	0.0316
	(0.002)	(0.022)	(0.001)	(0.011)	(0.005)	(0.019)
Treat	0.0003	0.0038	0.0011**	0.0044	-0.0117***	-0.0258*
	(0.002)	(0.014)	(0.000)	(0.009)	(0.004)	(0.015)
Accruals to Assets	0.4042***					
	(0.034)					
Log NOA		0.6881***				
		(0.094)				
R&D to Sales			0.9805***			
			(0.018)			
Log Employees				0.9817***		
				(0.005)		
Log Avg EE Pay					0.9515***	0.0284
					(0.008)	(0.027)
Log Avg Exec Pay					-0.0021	0.8184***
					(0.003)	(0.016)
Log Market Cap	-0.0007	0.3007***	-0.0003	0.0060	0.0077***	0.0410***
	(0.001)	(0.097)	(0.000)	(0.007)	(0.002)	(0.007)
Log Book to Market	0.0048***	0.2935***	0.0003	-0.0449***	-0.0063**	-0.0266**
	(0.002)	(0.101)	(0.000)	(0.009)	(0.003)	(0.011)
Sales Growth	0.0250***	0.1630***	0.0035***	0.0505**	0.0049	0.0448
	(0.006)	(0.050)	(0.001)	(0.020)	(0.010)	(0.042)
LT Debt to Equity	0.0003	0.2083***	-0.0001	0.0110**	-0.0024	-0.0122
	(0.001)	(0.068)	(0.000)	(0.006)	(0.002)	(0.009)
Cash to Equity	0.0125***	-0.2537***	0.0005	-0.0185**	-0.0036	-0.0319
	(0.002)	(0.098)	(0.000)	(0.009)	(0.004)	(0.020)
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	No	No	No	No	No	No
Observations	2,779	2,710	2,785	2,654	2,560	2,560
R^2	0.2295	0.9618	0.9260	0.9883	0.9209	0.7136

Table 9.

Market Valuation

Table 9 reports the results of a reduced form and instrumental-variables (IV) analyses using *Forward BM* as the dependent variable. The endogenous variable of interest in the IV analysis, *Forward ROE*, is instrumented for by our DID estimator, *Treat* x *Post*. As in Table 3, *Treat* is equal to one for firms ranked 301–500 under the JPX400 selection algorithm and zero for firms ranked 501–800; *Post* is an indicator variable assuming a value of one for years 2014–2016 and zero for the pre-treatment period, 2010–2012. Column 1 reports the results of a reduced-form DID effect estimate on *Forward BM*, similar to our main DID specification used in column 4 of Table 3. Columns 2 and 3 report the first and second stages of a IV regression. The last row of column 2 reports the first-stage F-statistic for the instrument. All firm-level control variables are defined in Table A1. Standard errors, clustered at the firm level, are reported in parentheses. Significance levels are indicated by *, **, *** for 10%, 5%, and 1% respectively.

	(1) OLS	(2) IV 1^{st} Stage	(3) IV 2^{nd} Stage
	Forward Log Book to Market	*Forward ROE*	*Forward Log Book to Market*
Forward ROE			-1.612*
			(0.91)
Treat x *Post*	-0.035*	0.022***	
	(0.02)	(0.01)	
Treat	0.032**	-0.004	0.025
	(0.01)	(0.01)	(0.02)
ROE	-0.093	0.407***	0.563
	(0.09)	(0.15)	(0.40)
Book to Market	0.854***	-0.038***	0.793***
	(0.02)	(0.01)	(0.04)
Log Market Cap	-0.026***	-0.013**	-0.047***
	(0.01)	(0.01)	(0.02)
Sales Growth	0.089**	0.009	0.104**
	(0.04)	(0.03)	(0.05)
LT Debt to Equity	-0.007	-0.006	-0.017
	(0.01)	(0.00)	(0.01)
Cash to Equity	-0.046***	0.012*	-0.027
	(0.01)	(0.01)	(0.02)
Time FE	Yes	Yes	Yes
Observations	2,725	2,725	2,725
R^2	0.7540	0.2845	0.6954
First Stage F		9.4818	